LETTERS TO THE EDITOR

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Temperature of the Body in Mountain Climbing

I HAVE read with great interest the able paper on the Temperature of the Human Body during Mountain Climbing in NATURE, vol. xii. p. 132, and as it is there mentioned that the results obtained by Drs. Marcet and Lortet require confirmation, I am tempted to send some extracts from notes of observations made on myself while on a walking tour last autumn, in the Yorkshire moors. I made in all five ascents, of heights over 2,000 feet, during all of which I took notes of my temperature at intervals. As, however, I had no more than a hearsay acquaintance with Dr. Marcet's results, and was not aware of the important influence of the act of ascending as distinguished from the elevation attained, my earlier results were not sufficiently connected to be worth publishing. Suffice it to say that I always obtained a fall of from one to two degrees. On the fifth day of observation, when I was alive to this and other sources of error, I made the following observations in the course of the ascent of Whernside and Gragreth.

	Time.	Height in feet.	Temperature in mouth.
In bed, Chapel le Dale, feeling warm	7.30	900	97·7
Breakfast	8.30	—	—
Before starting, feeling cold	9.40	—	97·6
Walked one mile nearly level; spent half an hour at Gate Kirk Cave, then a steep ascent; after rising 1,000 feet and while hot, tired, and sweating, and before stopping	11.20	1,900	96·4
Sat down; after ten minutes' rest felt fresh, and neither hot nor cold	11.30	—	98·2
Ascent continues steep till near the top, when it is moderate; reached the top hot, sweating, and out of breath; temp. of air 47°, barometer 27·4 in.	12	2,414	97·6
After sitting still in a cold wind and eating 4 oz. biscuits; toes and fingers cold, and shivering slightly	12.37	—	99·3
Steep descent of 1,000 feet, came down at a run; fingers and toes getting warm; before stopping	1.10	1,400	98·0
Crossed the valley to ascend Gragreth; after climbing 500 feet, sweating and feeling hot, and before stopping	2.17	1,900	96·4
After sitting seven minutes	2.24	—	97·6
Still sitting, feeling cooler	2.29	—	98·6
Still sitting	2.33	—	98·6
Nearly at the top	2.47	2,200	98
On the flat top of Gragreth, going slower, feeling cooler	2.52	2,250	98·2
Sitting, feeling cold, strong wind...	3.12	—	98·4
After descending 1,050 feet rapidly	3.55	1,200	98·4
After sitting ten minutes...	4.5	—	98
Sitting warm, at "George and Dragon" Inn, Dent...	9.50	500	97·9

In comparing the temperatures above it should be borne in mind that I uniformly found my temperature in bed in the morning 97·6 or 97·7, and about the same at 9 or 10 at night; while the day previous, when detained in the house by bad weather, it had been 98·4 and 98·6 in several observations, between 10.30 A.M. and 6 P.M., and this I have found the case on many other occasions, so that the difference between the second and third observations really represents a greater depression than is apparent.

It is worthy of note that the two lowest temperatures, viz., 96·4, both occurred during steep ascents and before stopping, and while I was perspiring freely and feeling hot. In each case I immediately sat down and noted a progressive rise in the temperature, though at the same time I was feeling much cooler. When I began to shiver from sitting above half an hour in a strong wind on the top of Whernside the temperature had actually risen to 99°·3. This reminds one of the cold stage of ague, when a patient may have a temperature of 105° while his teeth are chattering with cold.

I entirely agree with Dr. Marcet that it is the fact of actively climbing and not the actual elevation, which influences the temperature. When sitting quiet on the top of Whernside and Gragreth the temperatures were at or above the normal. The steepness also appears to have great influence; both these mountains have flattish tops, and the temperature on reaching the top of Whernside shows less depression than during the ascent, while that taken walking on the flat top of Gragreth, though without stopping, after the ascent showed scarcely any.

I noticed no depression of temperature in descending, though I often came down at a run, and none in several long level walks.

And now as to any possible fallacies. The same thermometer was used in all the observations. It is a Philips' maximum, by Wood and Co., of York, is graduated in fifths of a degree, and easily reads to tenths. I have compared it with a thermometer certified at Kew, and find it very exact. It rises to within a degree of the truth in ten seconds after putting under the tongue, and I am accustomed to rely on its indications after being in about two minutes. In all the above observations it was left in the same position under the tongue five minutes by the watch (except the reading at 2.33, when it was only four); consequently any failure to rise to the true temperature would be likely to affect all the readings nearly alike. Whatever error may be due to this cause would be likely to show itself in raising the readings during the ascents, as at these times the heart was beating strongly and the circulation particularly active; so that the cooling of the mouth by the cold bulb would be quickly neutralised. I was also particularly careful not to allow any air to enter through the lips and as little as possible through the fauces, and feel confident that there is no appreciable error from this cause.

As to the *rationale* of the process, may not the following theory embrace all the facts as observed by Marcet, Forel, and others?

Heat motion and chemical force are merely varieties of one force convertible under certain conditions. The human body is an engine, in which muscle and other tissues are oxidised, and the resulting chemical force is transformed into an equivalent amount of motion and heat. Now it is manifest that there is a limit to the rate at which oxidation can go on in the body, and consequently to the amount of force available for transformation in an unit of time into motion and heat. Usually the human machine is not worked up to its full power; the amount of motion produced is nothing approaching what is possible, and only sufficient force is transformed into heat to keep the body at the normal temperature of 98·4; but in the limit the two processes become complementary to each other, and if from any cause the force converted in one direction exceeds a certain amount, the excess can only be obtained at the expense of what would ordinarily be converted in the other. Now, in mountain climbing the amount of motion as expressed in foot-pounds of work done is very great, and much exceeds the amount to which most of us are accustomed in walking on a level road. If this be pushed to the limit it encroaches on the amount of force destined for the supply of heat to the body, and the temperature falls. It is clear that these processes habitually go on at different rates in different individuals; if so, may not the limit of the rate of tissue change be different? One man may be able to push his motion production nearly to the limit up to which his oxidation will work, thereby encroaching on what ought to sustain his temperature, while another, whose muscles are less fully under the dominion of his nerves, or whose power of oxidation is considerable, may be unable to reach that limit.

To this class I believe Dr. Forel to belong; in the other, the slow oxidisers, I find myself in the honourable company of Drs. Marcet and Lortet.

TEMPEST ANDERSON

17, Stonegate, York

Trevandrum Magnetic Observations

THOUGH I have felt much gratified with the notice by Prof. B. Stewart of the first volume of the "Trevandrum Observations" which appeared in last week's NATURE, p. 163, I desire, nevertheless, to make a few remarks on the only point on which we appear to differ.

There are two methods of investigating the laws of magnetic disturbance, which have quite distinct objects; one, which has been employed with much success by that eminent veteran of science Sir E. Sabine, seeks, as he has expressly stated, the laws of the *larger* disturbances only, and for this end chooses only those deviations from the mean positions that are greater than some arbitrary value. The other method seeks the laws of disturbances of *all* magnitudes, and employs deviations of *all* amounts.

As Prof. Stewart regrets that I have employed the latter method, and suggests that the former may yet be used by others,

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it may be supposed that the laws of disturbance are not found by me, and are not to be found by the method which I have employed. This would be a great mistake; one which I am bound to correct.

The method which Prof. Stewart recommends has had objections proposed to it by the Astronomer Royal, the Provost of Trinity College, Dublin, and by myself. It is, I think, to defend the method against these objections that Prof. Stewart has written his remarks on the modes of discussion; but I have never heard any objections to the other method, nor, as far as I can understand, does he offer any.

As the method followed by Dr. Lloyd on the Dublin Observations and by myself on the Makerstoun and Trevandrum Observations has shown every law of magnetic disturbance that has been obtained by the other, I am afraid I cannot see that the illustration of the cyclones is applicable to the two methods, even if we were bound to study large cyclones only and to put those of less than a given magnitude out of consideration.

4, Abercorn Place, London, N.W. JOHN ALLAN BROUN

Anomalous Behaviour of Selenium

IT has been lately observed that the electrical resistance of selenium is greater in the light than in the dark. It was at first thought possible that this increase of resistance might be due to heat admitted with the light, but Prof. W. G. Adams, in his paper read before the Royal Society, June 17th, 1875, has shown that this is not the case, but that the phenomenon is a purely optical one.

The writer of this letter has to-day tried an experiment with a selenium bar belonging to the Cavendish Laboratory. Its length is 50 mm., breadth 8 mm., thickness about 1 mm.; platinum wires are soldered to its ends, and it has a hard metallic surface. Its electrical resistance is enormous. In the dark it is just over 100 megohms (100,000,000 B.A.U.) When, however, the light of the paraffin lamp of the galvanometer was allowed to fall on it from the distance of about a foot, the resistance DECREASED between 20 and 30 per cent. The experiment was repeated many times, with current sent sometimes one way, sometimes another, and with different sides and edges of the bar turned to the light, but always with the same result, namely, that the effect of letting in the light was to largely *decrease* the resistance.

A second set of experiments were made with a selenium medal struck by Berzelius soon after the discovery of the metal in 1818, and presented by him to Mr. Deck, by whose son, Mr. Deck of Cambridge, it was kindly lent for the experiment. This medal was of oval shape, about 40 millims. long by 30 broad. Owing to the difference of form between the two specimens, their specific resistances could not be accurately compared; that of the medal was, however, not more than about $\frac{1}{10}$ of that of the bar. The medal was exactly like black lead both to touch and sight, and quite different in appearance to the bar. The resistance of the medal *was sensibly the same, both in the dark and in the light; no difference could be detected.*

These experiments seem to show that the physical form of the metal has a great deal to do with its behaviour when carrying an electric current and exposed to light.

J. E. H. GORDON

Cavendish Laboratory, Cambridge, June 29

The House-fly

AS no one more competent than myself seems disposed to reply to the query of "Harrovian" (NATURE, vol. xii. p. 126) respecting a disease of the house-fly, and which is again referred to by the Rev. D. Edwardes in last week's NATURE, I may perhaps be permitted to make a few remarks thereon.

I have frequently noticed dead and dying flies thus affected, generally in the late summer and autumn; and I think I am right in attributing the phenomenon to the growth of a parasitic fungus, called, I believe, *Empusa musci*, in the fly's body. The insects may often be seen settled in a natural position on window-panes, but with the abdomen much distended, and surrounded by a collection of whitish powder, extending for a few lines in all directions on the surface of the glass. The whole of the interior organs of the abdomen are consumed by the plant, nothing remaining but the chitinous envelope, on which the mycelia of the fungi form a felt-like layer; the fructification showing itself externally as filaments protruding from between the rings of the body.

Insects are very subject to the attacks of such parasites. Some of those living in the interior organs of the body seem to do little if any injury to their "hosts," while others completely destroy them; as in the case of *Sphæria*, which changes the caterpillars at whose expense it lives into a mass of fungoid growth of most grotesque appearance. It is now well ascertained that a species of *Botrytus* produces the dreaded "Muscardine" of the silk-growers; and every practical lepidopterist has had to lament the destruction of pet broods of larvæ by some similar disease, which, though perhaps sometimes pathological, is probably in the first instance set on foot by fungi.

The whole subject of the parasites of insects is extremely interesting. According to my experience it is the exception for an insect to be quite exempt from the attacks of one or more animal or vegetable entozoic or epizoic organism; and I have often found five or six different species inhabiting one unfortunate individual.

I may mention that "Harrovian" will find some remarks on this fly-fungus by Dr. Cohn, in an early volume of the *Journ. Micros. Science*. I regret that, writing away from home, I cannot give the exact reference. W. COLE

Stoke Newington, N., July 2

[We print this letter from among several which all correctly explain the phenomenon under consideration in a similar manner.—ED.]

Theories of Cyclones

IN NATURE, vol. xii. p. 98, you notice a paper by Dr. Hann on two rival theories of cyclones. According to one, "whirlwinds are formed mechanically by different streams of air meeting, and centrifugal force causes the central depression. The more modern theory regards a local depression as the first condition, causing an indraft resulting in a whirlwind through the earth's rotation: the primary depression is held to follow condensation of vapour."

The question is how the cyclone begins: whether the first depression is due to the centrifugal force of an eddy, or to the expansion of air in the upper strata from the heat liberated in the condensation of vapour. There need not be any controversy as to the dynamics of the cyclone after it is formed.

There is a mass of geographical evidence in favour of the first-named theory, namely, that cyclones originate in the conflict of the trade-winds of the northern and southern hemispheres when either trade-wind is drawn to some distance across the equator. (A cyclone cannot be formed on the equator, because there the earth has no rotation in relation to a vertical axis). On this subject see Mr. Meldrum's paper in NATURE, vol. ii. p. 151, and my letter in NATURE, vol. iv. p. 305; also Mr. Maury's paper in NATURE, vol. viii. pp. 124, 147, 164.

Mr. Maury fully recognises the truth that the motive power of the cyclone, once it is formed, consists in the heat liberated by the condensation of vapour, which causes expansion in the upper strata and produces an ascending current. I believe the nature of these actions was first explained by Espy, whose "Philosophy of Storms," though well known by name, seems to be less appreciated than it deserves.

There is, however, another reason for the existence of an ascending current at the centre of a whirlwind, which I do not think I have seen stated. The lowest atmospheric stratum of a whirlwind is retarded in its motion by friction against the earth, and its centrifugal force is thereby lessened in proportion to that of the upper strata. The effect of this relative deficiency of centrifugal force in the lowest stratum—that is to say, at the surface of the earth—must be to cause a flow of air at the surface of the earth towards the centre of the whirlwind, and an ascending current at its centre. Such an ascending current is probably the cause of the vertical columns of dust that accompany those small whirlwinds which are common in windy weather.

Old Forge, Dunmurry, JOSEPH JOHN MURPHY
Co. Antrim, June 23

The Dark Argus Butterfly

IT is stated in H. N. Humphrey's work on "British Butterflies," that the Dark Argus Butterfly appears in July, and has only been found in the neighbourhood of Durham and Newcastle, and seldom above half a mile from the sea. When in May I was at Ashmore, which is on the borders of Dorset and Wilts, I took some butterflies answering exactly to the description of

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